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A DUAL ESP EXPERIMENT
WITH CLOCK CARDS

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IN discussing the puzzling effects in the Fisk data of the Home-testing ESP experiments (1) D.J.W. suggested that G.W.F. seemed to belong to a group of successful experimenters while he himself belonged to a group who could only achieve null results. As G.W.F. had recently conducted successful trials with the Clock card technique, it was thought it might prove interesting if further trials were organized with G.W.F. and D.J.W. acting alternately as experimenters with the same subjects under the same conditions.

Design of the Experiment

(a) The same DTSP method was followed as in the first Clock card trials (2). Thirty-two sealed packs of twelve cards each were sent, in sets of four packs at a time, to each of twenty subjects who were asked to call the hour depicted on each card from top to bottom of the pack and to record their calls on the numbered score sheets provided. Sixteen of these packs were made up in random order by G.W.F. and sixteen by D.J.W.¹ All packs were posted to subjects by G.W.F. and returned, unopened, to him. Subjects did not know that half of these packs were being made up by D.J.W. and if they had known they would not have been able to tell which were which.

(b) The order in which the subjects were given the two kinds of packs was randomized by means of 8×8 Latin Squares.

(c) G.W.F. and D.J.W. each checked the scores against the targets in their own sections and did not see each other's targets until the experiment was over. Score sheets and the whole of the

¹ One subject (J.C.) inadvertently guessed the same pack twice. This was an isolated error and his results have been included in the total.

data were independently re-checked with the help of Miss A. Nangie.

(d) With the exception of two or three runs at the beginning of G.W.F.'s section, a written record of the target random order was kept and regarded as the true final target. This convention excluded the possibility of recording errors correlated with the subject's guesses.

(e) The data are available for inspection and further analysis, e.g. for 'decline effect', 'target reinforcement effect', etc.

Results

The scores were assessed by two methods of scoring, (A) Direct Hits only, and (B) Differential Scoring by the Hour Divergence method (2). Full details are given in Appendices I and II of this report. We here summarize the main points.

(A) DIRECT HITS

(a) The combined scores of all the twenty subjects give a significant positive result. There is a probability of one in 900 times approximately that the deviation of the size obtained would arise by chance, or, to put it another way, one would expect a comparable result to occur by chance only once in about 900 similar experiments.

(b) The totalled score of D.J.W.'s section is insignificant. The combined significant total is due to the scores in G.W.F.'s section with a probability of one in over 6000.

(c) This suggests that D.J.W. is a jinx and gets only null results, for the scores of his section have only diluted the better results obtained when G.W.F. was experimenter. But that is by no means the whole story.

(d) Still considering the combined scores of both sections, three subjects, Dr B., C.R., and S.M. give individually significant, positive results. S.M.'s score is very outstanding (probability in the region of one in a million) and very clearly significant notwithstanding that she is selected as the best among twenty subjects. Moreover, S.M. was the subject who gave outstanding scores in previous ESP tests (see Appendix VIII). But the most interesting point is that S.M. gave highly significant results with D.J.W.'s targets. Thus, whatever may have been the effect of D.J.W. as experimenter upon the other subjects, he certainly did not inhibit this star.

(e) Dr B., one of the other two subjects who produced a possibly significant score (probability about one in a hundred), had also previously given positive results in ESP/PK tests (3). The other high-scoring subject, C.R., had had no previous trials.

(f) The score of J.B. is interesting. J.B. (G.W.F.'s granddaughter) had taken part in the original Clock card experiments without much success. (J.B. is the younger sister of S.B. who gave the first significant results with the new technique.) J.B. gave an indisputably significant result with G.W.F. (probability about one in 1400)¹ but made a negative score with D.J.W. which reduced her combined total below the level of significance. She, unlike S.M., thus reacted differently to the two sets of cards. It must be noted that towards the end of the experiment, with a perspicacity impossible to elude, she had tumbled to the fact that D.J.W. had had a hand in preparing some of the packs. However, in the earlier part of the test, before she realized there was a second experimenter, her scores showed the same trend.

(B) DIFFERENTIAL SCORES

The differential scores give some credit for near misses but they also take into account direct hits, so one would expect that their general trend would be the same as with the results already considered.

(a) The combined scores give a positive result bordering on significance (probability 1 in 23).

(b) The total score of D.J.W.'s section shows a negative deviation and is quite insignificant. G.W.F.'s section total is positive and significant (probability about one in 1600).

(c) Dr B. and S.M. are the only two subjects to give unmistakably significant results. Dr B.'s probability works out at one in 1150 and S.M.'s is one in more than a thousand million. So S.M. is still the star with high scores with both G.W.F. and D.J.W., with probabilities of the order of one in a hundred thousand in both cases.¹ For the rest, J.B., again made a good positive score with G.W.F. but a negative score with D.J.W.

Conclusions

(a) With several of the subjects Clock card tests continue to yield positive results.

(b) The outstanding subject S.M. produces significant scores with *both* experimenters.

(c) The figures suggest that G.W.F. is chiefly responsible for the significant positive scores, and the question arises whether the difference in the total scores of the two sections reflects a general trend throughout the data or only the difference in behaviour of a

¹ These probabilities are only approximate because with a largish *t* value and a relatively small sample the normal distribution is not strictly applicable.

few exceptional subjects. There is hardly sufficient data to answer this conclusively, and the *t* tests for the significance between the means of the two scores (see Appendix VII), although yielding positive results, are not sufficiently convincing to warrant any firm conclusion. Probabilities are approximately one in 8 and one in 33 for Direct Hits and Differential scores respectively. Thus, although at first sight the outcome of the experiment is striking, there is obvious need for further research.

That need becomes even more imperative when one considers the very slender contact the experimenter has with the subject in the DTSP form of tests. The fact, if fact it be, that the person who actually prepares the lists of random numbers, arranges the cards in the packs, and finally assesses and marks the score sheets, should have some influence on the result is very mysterious and surprising. The random numbers are picked out and listed several hundreds at a time and packs prepared and sealed in advance. The experimenter cannot know normally which cards are sent to which subject, and yet, apparently, the subject's psi faculties somehow are affected and work better with Experimenter A's packs or are inhibited by the packs of Experimenter B.

Call Frequencies

In previous Clock card tests it was found that subjects showed very decided preferences for certain of the hours on the clock face. Appendix III shows the actual number of calls by each subject for each of the twelve hours. The preferred hours (in order of popularity) are :

I, IX, V, VI, II, VII.

The unfavoured hours (in order of distaste) are :

XII, X, XI, VIII.

In the first experiments (2) the preferred hours were :

II, VII, III, IX, IV, VI, I.

and the following disliked :

XI, XII, VIII, X, V.

Viewing all results to date, therefore, the subjects are more consistent in their dislikes than in their likes.

Emphatically disliked are :

XII, XI, X, VIII.

Consistently liked are :

I, IX, II, III.

Perhaps the psychologists could tell us why.

Target Frequencies

Appendix IV shows the number of appearances of each of the twelve targets (I–XII) and was compiled as a check on the randomization. It will be seen that the distribution, although not ideally regular, does not vary significantly from the norm. The deviation becomes proportionately smaller as the number of trials (as in the combined totals) becomes greater.

Cross-checks

Appendices V and VI give the results of cross-checks in which the calls of one subject are scored against the targets supplied to the *next* subject. The totals of both sections, singly or combined, give only chance results. Of the individual scores there were two deviations, one positive and the other negative, for which the *t* values were as much as 2.40. But when it is remembered that these were picked from among 20 different subjects with two methods of scoring, it will be seen these figures come well within the range of mean chance expectation. The comparison of these cross-check scores with those of the actual results is indeed impressive, and suggests very strongly that we are not merely seeing the effects of some statistical artifact.

REFERENCES

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APPENDIX I
DIRECT HITS SCORES

Sub- ject	FISK TARGETS			WEST TARGETS			COMBINED		
	Dev.	<i>t</i>	<i>P</i>	Dev.	<i>t</i>	<i>P</i>	Dev.	<i>t</i>	<i>P</i>
J.C.	-3	-0.78		-6	-1.57		-9	-1.66	
J.B.	13	3.39	0.0007*	-6	-1.57		7	1.29	
S.B.	10	2.61	0.009	-1	-0.26		9	1.66	
Dr B.	12	3.13	0.002	2	0.52		14	2.59	0.01
P.C.	1	0.26		0	0		1	0.18	
E.D.	4	1.04		-5	-1.31		-1	-0.18	
C.R.	10	2.61	0.009	4	1.04		14	2.59	0.01
I.F.	5	1.31		0	0		5	0.92	
E.W.	0	0		0	0		0	0	
I.J.	7	1.83		3	0.78		10	1.85	
C.L.	-1	-0.26		1	0.26		0	0	
B.O.	6	1.57		0	0		6	1.11	
G.R.	-5	-1.31		-1	-0.26		-6	-1.11	
A.W.	2	0.52		3	0.78		5	0.92	
S.M.	8	2.09	0.04	20	5.22		28	5.17	10 ⁻⁷
J.W.	-6	-1.57		1	0.26		-5	-0.92	
S.G.	1	0.26		-4	-1.04		-3	-0.55	
D.J.	-1	-0.26		0	0		-1	-0.18	
H.G.	0	0		4	1.04		4	0.74	
K.G.	2	0.52		-1	-0.26		1	0.18	
	65	3.79	0.00015	14	0.82		79	3.26	0.0011

With S.M.'s scores omitted the figures are :

57 3.41 0.0065 -6 -0.36 51 2.14 0.032

* In this and in Appendix II the probabilities associated with individual subjects are only approximate because, with relatively small samples and largish *t* values the normal distribution is not strictly applicable.

APPENDIX II

DIVERGENCES SCORES

Sub- ject	FISK TARGETS			WEST TARGETS			COMBINED		
	Dev.	<i>t</i>	<i>P</i>	Dev.	<i>t</i>	<i>P</i>	Dev.	<i>t</i>	<i>P</i>
J.C.	-28	-1.13		-44	-1.78		-72	-2.06	0.039
J.B.	81	3.28	0.001	-54	-2.19	0.03	27	0.77	
S.B.	33	1.34		-12	-0.49		21	0.60	
Dr B.	84	3.41	0.0006	32	1.30		116	3.33	0.0009
P.C.	40	1.62		-19	-0.77		21	0.60	
E.D.	35	1.42		-23	-0.93		12	0.34	
C.R.	56	2.27	0.02	-8	-0.32		48	1.38	
I.F.	-33	-1.34		5	0.20		-28	-0.80	
E.W.	-17	-0.69		-27	-1.10		-44	-1.26	
I.J.	16	0.65		2	0.08		18	0.52	
C.L.	6	0.24		6	0.24		12	0.34	
B.O.	57	2.31	0.02	18	0.73		75	2.15	0.03
G.R.	-24	-0.97		-6	-0.24		-30	-0.86	
A.W.	-19	-0.77		-25	-1.01		-44	-1.26	
S.M.	110	4.46	10 ⁻⁵	117	4.75	10 ⁻⁶	227	6.51	10 ⁻¹⁰
J.W.	7	0.28		6	0.24		13	0.37	
S.G.	27	1.09		-12	-0.49		15	0.43	
D.J.	-22	-0.89		11	0.44		-11	-0.32	
H.G.	-25	-1.01		16	0.65		-9	-0.26	
K.G.	-7	-0.28		-44	-1.78		-51	-1.46	
	377	3.42	0.0006	-61	-0.55		316	2.02	0.04

With S.M.'s scores omitted the figures are :

267 2.48 0.013 -178 -1.66

89 0.57

APPENDIX III

CALL FREQUENCIES, FISK SECTION

	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII
J.B.	13	16	16	23	8	10	20	23	39	5	14	5
S.B.	20	11	18	18	15	20	17	14	20	16	16	7
Dr B.	17	17	18	15	26	14	12	13	13	17	20	10
J.C.	13	17	18	11	20	21	17	23	17	7	13	15
P.C.	26	17	12	14	21	20	12	10	17	22	6	15
C.R.	27	10	12	6	13	21	22	7	21	10	19	24
E.D.	20	17	17	15	15	18	16	16	18	16	10	14
I.F.	16	12	21	15	22	9	25	18	18	12	14	10
S.G.	23	18	19	12	14	24	15	13	14	14	12	14
K.G.	22	18	12	13	16	10	17	16	16	17	19	16
H.G.	19	25	13	18	20	17	4	18	20	8	15	15
D.J.	12	17	20	19	23	25	16	14	26	8	4	8
I.J.	14	14	20	14	12	18	20	15	19	17	17	12
C.L.	13	16	17	17	19	20	24	5	20	11	17	13
B.O.	16	14	16	16	14	18	17	20	19	11	16	15
G.R.	17	18	16	20	22	18	11	24	21	6	13	6
S.M.	21	22	18	10	22	12	22	8	13	16	20	8
E.W.	22	22	17	11	15	14	13	17	19	20	10	12
A.W.	26	15	7	22	18	7	17	17	16	18	17	12
J.W.	17	17	18	20	15	22	13	14	10	17	14	15
Totals	374	333	325	309	350	338	330	305	376	268	286	246
<i>d</i>	54	13	5	-11	30	18	10	-15	56	-52	-32	-74
<i>d</i> ²	2916	169	25	121	900	324	100	225	3136	2704	1156	5476

$$\text{Sum } d^2 = 17252 \quad \chi^2 = 17252/320 = 53.91 \text{ (11 d.f.)} \quad P \approx < 7 \times 10^{-9}$$

APPENDIX III (continued)

CALL FREQUENCIES, WEST SECTION

	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XI
J.B.	26	21	9	24	5	11	16	22	42	3	10	3
S.B.	16	16	14	18	16	15	17	12	24	17	19	8
Dr B.	16	18	21	16	21	15	16	12	16	17	13	11
J.C.	16	15	21	9	22	19	17	20	18	10	10	15
P.C.	23	13	10	14	22	21	19	14	14	21	8	13
C.R.	22	18	14	5	12	24	23	5	23	10	17	19
E.D.	19	18	17	16	18	14	16	17	13	15	13	16
I.F.	21	17	10	15	18	11	15	18	16	26	15	10
S.G.	17	16	18	17	17	25	13	14	15	18	8	14
K.G.	22	20	11	14	13	18	14	17	12	17	19	15
H.G.	18	18	15	16	16	15	9	21	22	12	13	17
D.J.	17	16	17	18	27	19	19	16	22	10	8	3
I.J.	23	14	21	14	12	22	18	13	18	10	13	14
C.L.	17	16	17	16	21	24	20	4	17	13	17	10
B.O.	14	19	16	17	18	14	21	17	15	12	17	12
G.R.	16	14	18	15	18	23	16	23	20	5	15	9
S.M.	14	12	19	15	18	19	21	7	17	13	19	16
E.W.	18	19	11	24	16	12	12	22	17	17	11	13
A.W.	18	17	14	21	14	11	20	19	9	18	16	15
J.W.	16	15	21	19	18	21	9	11	11	15	23	13
Totals	369	332	314	323	342	353	331	304	361	281	284	246
d	49	12	-6	3	22	33	11	-16	41	-39	-36	-74
d^2	2401	144	36	9	484	1089	121	256	1681	1521	1296	5476
Sum $d^2 = 14514$ $\chi^2 = 14514/320 = 45.36$ (11 d.f.) $P \approx 10^{-4}$												

FISK AND WEST SECTIONS COMBINED

d	103	25	-1	-8	52	51	21	-31	97	-91	-70	-146
d^2	10609	625	1	64	2704	2601	441	961	9409	8281	4900	21904
Sum $d^2 = 62500$ $\chi^2 = 62500/640 = 97.66$ (11 d.f.) $P = \text{extremely small}$												

APPENDIX IV

TARGET FREQUENCIES, FISK SECTION

	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII
J.B.	14	14	20	23	14	12	15	17	16	18	16	13
S.B.	17	14	20	17	14	21	19	11	17	11	18	13
Dr B.	22	15	18	23	14	12	17	17	15	12	16	11
J.C.	17	12	20	12	16	21	14	12	18	17	21	12
P.C.	16	15	22	19	16	15	13	19	12	18	11	16
C.R.	20	13	21	17	14	16	17	13	16	18	16	11
E.D.	14	12	9	9	9	21	23	17	22	22	16	18
I.F.	18	13	20	20	7	25	19	12	14	13	11	20
S.G.	14	13	20	17	19	25	18	10	24	10	15	7
K.G.	15	12	12	17	17	19	19	21	16	11	18	15
H.G.	18	15	18	10	8	22	10	20	17	19	19	16
D.J.	12	12	21	17	14	17	23	9	23	18	12	14
I.J.	19	16	19	18	11	15	11	13	28	12	16	14
C.L.	18	12	12	18	17	17	15	26	19	16	13	9
B.O.	16	15	14	13	16	13	17	20	13	16	17	22
G.R.	12	16	15	12	13	20	17	13	16	12	17	29
S.M.	23	18	11	20	13	15	16	13	15	22	12	14
E.W.	16	17	16	9	18	17	14	9	12	22	17	25
A.W.	22	19	13	16	22	15	16	9	21	16	12	11
J.W.	18	19	18	14	13	14	15	14	17	14	19	17
Totals	341	292	339	321	285	352	328	295	351	317	312	307
d	21	-28	19	1	-35	32	8	-25	31	-3	-8	-13
d^2	441	784	361	1	1225	1024	64	625	961	9	64	169
Sum $d^2 = 5728$ $\chi^2 = 5728/320 = 17.9$ (11 d.f.) $P \approx 0.1$												

APPENDIX IV (continued)

TARGET FREQUENCIES, WEST SECTION

	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII
J.B.	14	11	12	18	15	17	17	16	15	17	19	21
S.B.	19	20	12	7	20	17	15	20	18	17	12	15
Dr B.	13	15	17	15	13	15	21	25	19	12	12	15
J.C.	17	13	22	20	12	11	10	14	13	22	20	18
P.C.	19	13	13	14	27	15	18	16	18	11	16	12
C.R.	18	20	14	12	14	18	24	12	16	18	9	17
E.D.	10	16	10	29	9	18	19	20	17	8	18	19
I.F.	19	16	16	15	17	17	7	23	21	11	14	16
S.G.	10	14	25	15	18	16	16	23	14	11	13	17
K.G.	12	22	12	23	15	15	14	12	14	18	18	17
H.G.	18	17	12	18	20	10	14	19	17	10	14	23
D.J.	19	13	18	15	16	11	13	12	19	18	20	18
I.J.	15	15	14	23	19	17	8	20	16	12	11	22
C.L.	13	13	13	16	18	11	20	19	14	14	23	18
B.O.	14	13	22	21	14	18	13	16	16	9	20	16
G.R.	15	15	21	13	16	14	14	19	20	20	15	20
S.M.	11	16	21	13	18	15	10	16	15	13	20	24
E.W.	24	14	22	19	18	12	14	14	13	11	17	14
A.W.	15	12	15	17	16	13	14	16	23	20	19	12
J.W.	14	18	17	23	12	14	25	12	8	12	18	19
Totals	309	306	328	345	327	294	306	334	326	284	328	353
d	-11	-14	8	25	7	-26	-14	14	6	-36	8	33
d^2	121	196	64	625	49	676	196	196	36	1296	64	1089
Sum $d^2 = 4608$ $\chi^2 = 4608/320 = 14.40$ (11 d.f.) $P \approx 0.2$												

FISK AND WEST SECTIONS COMBINED

d	10	-42	27	26	-28	6	-6	-11	37	-39	0	20
d^2	100	1764	729	676	784	36	36	121	1369	1521	0	400
Sum $d^2 = 7536$ $\chi^2 = 7536/640 = 11.78$ (11 d.f.) $P \approx 0.3$												

APPENDIX V
CROSS-CHECKS : DIRECT HITS SCORES

Subject	FISK TARGETS			WEST TARGETS			COMBINED		
	Dev.	<i>t</i>	<i>P</i>	Dev.	<i>t</i>	<i>P</i>	Dev.	<i>t</i>	<i>P</i>
J.C.	-3	-0.78		2	0.52		-1	-0.18	
J.B.	-2	-0.52		-4	-1.04		-6	-1.11	
S.B.	2	0.52		2	0.52		4	0.74	
Dr B.	0	0		2	0.52		2	0.37	
P.C.	7	1.83		-2	-0.52		5	0.92	
E.D.	-1	-0.26		-2	-0.52		-3	-0.55	
C.R.	-4	-1.04		-3	-0.78		-7	-1.29	
I.F.	-1	-0.26		1	0.26		0	0	
E.W.	9	2.35	0.019	4	1.04		13	2.40	0.016
I.J.	-7	-1.83		-6	-1.57		-13	-2.40	0.016
C.L.	1	0.26		-2	-0.52		-1	-0.18	
B.O.	0	0		-1	-0.26		-1	-0.18	
G.R.	-2	-0.52		4	1.04		2	0.37	
A.W.	2	0.52		-6	-1.57		-4	-0.74	
S.M.	-4	-1.04		-2	-0.52		-6	-1.11	
J.W.	4	1.04		-2	-0.52		2	0.37	
S.G.	0	0		1	0.26		1	0.18	
D.J.	-2	-0.52		1	0.26		-1	-0.18	
H.G.	-1	-0.26		0	0		-1	-0.18	
K.G.	7	1.83		-6	-1.57		1	0.18	
	5	0.29		-19	-1.11		-14	-0.58	

APPENDIX VI
CROSS-CHECKS : DIVERGENCES SCORES

Subject	FISK TARGETS			WEST TARGETS			COMBINED		
	Dev.	<i>t</i>	<i>P</i>	Dev.	<i>t</i>	<i>P</i>	Dev.	<i>t</i>	<i>P</i>
J.C.	-21	-0.85		10	0.40		-11	-0.32	
J.B.	-10	-0.40		21	0.85		11	0.32	
S.B.	23	0.93		22	0.89		45	1.29	
Dr B.	-11	-0.45		13	0.53		2	0.06	
P.C.	-10	-0.40		-20	-0.81		-30	-0.86	
E.D.	-9	-0.36		-5	-0.20		-14	-0.40	
C.R.	-2	-0.08		-16	-0.65		-18	-0.52	
I.F.	-17	-0.69		12	0.49		-5	-0.14	
E.W.	5	0.20		8	0.32		13	0.37	
I.J.	-32	-1.30		-49	-1.99	0.05	-81	-2.32	0.02
C.L.	-8	-0.32		-18	-0.73		-26	-0.75	
B.O.	-10	-0.40		-1	-0.04		-11	-0.32	
G.R.	13	0.53		46	1.87		59	1.69	
A.W.	11	0.45		-25	-1.01		-14	-0.40	
S.M.	8	0.32		-28	-1.13		-20	-0.57	
J.W.	20	0.81		-13	-0.53		7	0.20	
S.G.	-13	-0.53		-7	-0.28		-20	-0.57	
D.J.	17	0.69		21	0.85		38	1.09	
H.G.	10	0.40		0	0		10	0.29	
K.G.	16	0.65		6	0.24		22	0.63	
	-20	-0.18		-13	-0.12		-33	-0.21	

APPENDIX VII

t test for significance of difference between
means of scores of G.W.F.'s and D.J.W.'s sections

Subject	DIRECT HITS		DIVERGENCES	
	Dev. (F - W)	(F - W) ²	Dev. (F - W)	(F - W) ²
J.C.	3	9	16	256
J.B.	19	361	135	18225
S.B.	11	121	45	2025
Dr B.	10	100	52	2704
P.C.	1	1	59	3481
E.D.	9	81	58	3364
C.R.	6	36	64	4096
I.F.	5	25	-38	1444
E.W.	0	0	10	100
I.J.	4	16	14	196
C.L.	-2	4	0	0
B.O.	6	36	39	1521
G.R.	-4	16	-18	324
A.W.	-1	1	6	36
S.M.	-12	144	-7	49
J.W.	-7	49	1	1
S.G.	5	25	39	1521
D.J.	-1	1	-33	1089
H.G.	-4	16	-41	1681
K.G.	3	9	37	1369
	51	1051	438	43482

Student's *t* (19 d.f.) *t* = 2.32
= 1.6 *P* ≅ 0.03
Insignificant.

APPENDIX VIII

Summary of all S.M.'s Clock card scores to date (September 1953)

Total runs = 52 = 624 trials
Direct Hits Deviation = +48 *t* = 6.95 *P* ≅ one in 3.0 × 10¹¹
Divergences Deviation = +374 *t* = 8.41 *P* ≅ one in 3.0 × 10¹⁶

COINCIDENCE AND SYNCHRONICITY

BY ANTONY FLEW

IN his review of C. G. Jung's *Naturerklärung und Psyche* Professor H. H. Price introduced readers of this *Journal* (Vol. XXXVII, No. 673) to that author's concept of synchronicity. Since we are certainly going to hear more of this notion, I want to add to the criticisms made already by Price. I should like here to thank Dr C. A. Meier of the C. G. Jung Institute at Zurich for help, both through his paper and through private discussion at Utrecht, towards understanding Jung's ideas: though of course he is not responsible for what I say.

(1) *Coincidence*. Coincidences, at least in the sense in which we speak of 'the long arm of coincidence' or 'any resemblance to actual persons living or dead is purely coincidental', are necessarily both 'non-causal' and 'meaningful'. That is to say: both that a pair or set, however remarkable, of occurrences cannot be said to be a coincidence or a series of coincidences if its members have some *common* causal origin; and that no pair or set of occurrences, however unconnected the circumstances which led up to them, can be correctly called a coincidence or a series of coincidences unless there is something remarkable about their coming together. Thus the *Concise Oxford Dictionary* explains 'coincidence' as '*notable occurrence of events or circumstances without apparent causal connection*' (my italics). Concretely, suppose a patient under analysis has had a dream in which someone has given her a golden scarab (a costly piece of jewellery). While she is telling this to her analyst he hears something gently tapping the window behind him, opens it, catches the insect which was making the noise and finds that it is a scarabeide, *Cotonia aurata*, the common rose bug whose green gold colouring most nearly resembles that of a golden scarab. This is precisely the sort of concurrence of events which is correctly called a coincidence: because it seems very strange and is the sort of thing that we want to retail to friends as 'the funny thing that happened today'; and insofar as we are sure that the two occurrences are not causally connected. It would not have been a coincidence had the analyst staged the arrival of the insect as part of the treatment: 'to puncture the hole we had been looking for in the thick armour of her rationalism and to break the ice of her intellectual resistance so that the treatment could now be continued with satisfactory results.' Nor would a concurrence of events be a coincidence if it seemed pointless, not the sort of thing to make one say 'very

strange' or want to tell the story. If the interruption in this case had been just any interruption—say a road accident outside—there would have been no coincidence. (Though people have been known to offer as psychic cases stories of pointless concurrences which could not even correctly be dignified by the name of coincidence.)

(2) *Synchronicity*. In a *synchronicity phenomenon* two or more events are linked together 'in a meaningful manner' (*sinngemäß*): though there is not, and indeed cannot be, any causal explanation for their concurrence, whether or not there is one for each event taken separately. Synchronicity is not just synchronousness: because it has this 'meaningfulness', which may sometimes amount to something uncanny or numinous; and because the events linked may not be strictly simultaneous, for the concept is intended to cover even 'precognitive' ψ . The suggestions are: that these synchronicity phenomena are somehow connected with the *archetypes* of the *collective unconscious* (two further Jungian technical terms); that they occur more commonly and less arbitrarily than they should 'by the law of averages'; and that this new concept may provide a key to the explanation of ψ in non-causal terms, since any causal explanation is said to be inconceivable.

Yet it is hard to see that this concept is either new or even potentially explanatory. Jung speaks of synchronicity as 'pointed' coincidence (*sinnvolle Coinzidenz*) and of events in a synchronicity phenomenon as linked in a 'meaningful' manner (*sinngemäß*): as if he were introducing a new species of coincidence. But to say that all (or some) coincidences are 'pointed' or 'meaningful' is to utter a tautology (or a ludicrous understatement): like saying all (or some) husbands are married. Synchronicity is not a (new) *species* of coincidence: it *is* coincidence. (Unless of course the three suggestions *about* synchronicity phenomena are to be built in as *part of the meaning of* 'synchronicity'.) That is why we were able to illustrate the meaning of the word 'coincidence' with one of the very cases Jung in his book (p. 22) gives as an example of synchronicity. And to say that something is (just a) coincidence is not any sort of explanation: but is in part to deny the possibility of explanation.

(3) *Causes and Archetypes*. Yet to leave it at that would be unfair, slick, and superficial: for though 'synchronicity phenomenon' may be merely new jargon for the old 'coincidence', still very substantial claims are being made in addition to or in the use of this neologism. Those of most interest to readers of this

journal are two : first, that—in spite of degrees of statistical significance which make us feel sure that ψ correlations are ‘more than coincidences’—these cannot conceivably be interpreted in causal terms ; and, second, that the notions of archetypes and of ψ can be brought fruitfully together. These are both subjects too enormous to be adequately treated here.

Two points nevertheless : (a) Such interpretation is alleged to be inconceivable : because ψ seems to be at least partially independent of space and time (and the notion of cause is inseparably bound up with those of space and time) ; and because there seems to be no room for energetic transactions in ψ (and causal transactions are always ‘energetic’, in the physicist’s sense). To say this is perhaps to go considerably beyond the facts so far established : and is certainly, as Price points out, to write about the notion of cause ‘as if Hume had never existed’. But even granted that interpretation in causal terms—whether those of Jung’s ‘very narrow and rigid conception’ (Price), or of what he rather dyslogistically calls ‘magical causation’, or of what Hume took to be the everyday notion—could be ruled out definitively : still that would not eliminate the possibility that our statistically significant ψ correlations were an indication, not of causal correlations waiting to be discovered, but of the possibility of finding laws which are functional only. A functional law is one which says that x varies with y (and any number of other variables) in accordance with some formula. Such a law is not necessarily, though it may be, interpretable in causal terms : in terms, that is, of causes which must always *precede* their effects—to say nothing of the more controversial requirement that causes must influence their effects *directly*, with no spatial or temporal gap intervening. As Russell long since rather tendentiously pointed out, already the laws of ‘the advanced sciences’ are functional and have no use for ‘The Notion of Cause’ (*vide* his *Mysticism and Logic*, 1918 ; Pelican Books, 1953). So even if we were to accept the contention that this notion has no possible place here, still we do not thereby have to concede that there are no functional laws to be found. And insofar as Dr Betty Humphrey, Dr Gertrude Schmeidler, and others have been successful in correlating ψ success with personality types, this process has already begun. (Though any theory which is to explain any such laws which are found will no doubt have to introduce novelties which will be attacked as inconceivable.)¹ Talk of ‘non-causal synchronicity’ would thus seem a premature counsel of despair.

¹ On theories and laws see J. J. C. Smart in *Logic and Language* II, Ed. : A. G. N. Flew (Blackwell, 1953), and S. E. Toulmin *The Philo-*

(b) The suggestion that ψ occurrences can be interpreted in terms of the archetypes would perhaps be more stimulating to the outsider if only he could feel sure that there was any (logical) possibility the realization of which could not be thus accommodated by the impressive ingenuity of Jungian analysts. For an 'explanation' which can be fitted to any (logically) possible occurrence is scarcely an explanation at all; since it does not tell us why, as a matter of contingent fact, things stand thus and thus, and *not otherwise*. To understand Jung's suggestion and—partly owing to the language difficulties which make the early appearance of the English translation most desirable, I cannot claim to have succeeded as fully as I should wish—it is essential to remember that however regularly he uses causal words of archetypes—speaking of their *activities* in *constellating* and *arranging* and so on—he definitely does not want us to think of them as the *causes* of non-causal synchronicities. But rather as what provides a context and helps to give point and meaning to them. Indeed it seems that the function of the archetypes is not causally to explain coincidences (which, by definition, cannot be done). Nor yet to explain why, in ψ , 'coincidences' occur far more often than they should (which, at least in what is taken to be the accepted scientific sense of 'explanation', is alleged to be inconceivable). But rather to show why certain concurrences are found striking, pointed, meaningful, even numinous; and subsumed under the concept of coincidence, while others are not. And this is indeed a possible psychological enquiry, in which psychological concepts such as *archetype* (provided they are impervious to at least some of the things which *might* happen, even if as a matter of fact they do not) may have an important place. But of course it is not at all the enquiry which has fascinated students of ψ . What laws can describe ψ phenomena: and can these phenomena be explained and controlled?

THE FIRST INTERNATIONAL CONFERENCE OF PARAPSYCHOLOGICAL STUDIES

The Conference held in Utrecht from 30 July to 5 August 1953 was a landmark in the history of psychical research. No organized discussions on an international basis had taken place since the Fifth International Congress for Psychical Research was held in Oslo in 1935, since when new lines of inquiry have been developed and many new workers have entered the field. The lack of personal contact was consequently becoming a serious matter.

sophy of Science (Hutchinson, 1953). And the latter too on 'cause' as a diagnostic notion. On the possible revision of the notion of cause see my *A New Approach to Psychical Research* (Watts, 1953), Ch. IX.

Psychical research has always been hampered by financial stringency, but since the war currency restrictions have made travel even more difficult. As in so many other fields of human endeavour, it was American vision and generosity which came to the rescue, and psychical researchers all over the world owe a debt of gratitude to Mrs Eileen Garrett and to the Parapsychology Foundation, New York, of which she is Founder and President, for making the Utrecht Conference possible.

The Conference, whose Chairman was Dr Gardner Murphy, was attended for the most part by specialists—psychologists, philosophers, physicists, physiologists, psychiatrists, engineers, mathematicians, etc.—who met ‘to review the situation of parapsychology in 1953, examine the research findings, and consider possibilities of future co-operative research planning’. Unlike the five International Congresses, it was attended by invitation only and was not open to the public.

After preliminary sessions, four working groups were established: the first dealing with quantitative experimental studies; the second with the interpretation of material gathered in the practice of psychiatry; the third with qualitative and spontaneous phenomena; and the fourth with the psychological study of persons who appear to display paranormal powers. The papers read, and the subsequent discussions, were simultaneously interpreted into English, French, and German, the official languages of the Conference, all discussions being recorded on tape recorders. Through the courtesy of the Curatorium of the University of Utrecht, the meetings were held in the University. In all, fifty-seven papers were presented. The question of publication is under discussion. One possibility which is being explored is that a general account of the Conference, with full details of its members and a complete, list of papers presented, should be circulated as soon as possible to all who attended, and that a selection of the papers, revised by their authors, should be published later in a series of monographs. The Conference established a Secretariat and a Publications Centre at the University of Utrecht, and Committees were charged with arranging subsequent meetings of groups interested in particular aspects of psychical research.

The feelings of all who attended this highly successful Conference are well expressed in the following Resolution which was passed with enthusiasm at the final Plenary Meeting:

The members of this Conference share a tremendous burden of gratitude, which no formal resolution can adequately relieve. Collectively we have attended many hundreds of conferences, but the occasion is rare, indeed, when we encounter either such hospitality, or

such efficiency, or such generosity, or such a combination of achievement with enjoyment. On this occasion, these have all been united in the most remarkable and felicitous way, and it has troubled us all throughout the period of this Conference that there is no obvious means by which we can repay these outstanding contributions. Perhaps we may hope that those to whom we now offer our thanks will feel best repaid in the knowledge of a joint achievement, and our individual determination that we shall in the future seek to justify the faith in us, which is implied by our presence here. We therefore resolve that our thanks be placed on record, first to Mrs Garrett, the Honourable Mrs Bolton, and the Parapsychology Foundation, to whose inspiration and extraordinary generosity the whole of this farsighted enterprise is entirely due; secondly, to the Executive Director of the Foundation, Dr Michel Pobers, whose unlimited energy and diplomacy have been our universal envy; to Mr Zorab, who has worked extremely hard in the most unassuming and unselfish way to help us, and finally to the Dutch Reception Committee, headed most efficiently by Professor Fischer, who has made us all so welcome here in this beautiful country, and in the town whose University has set a courageous example, which it will be our aim to make the general rule.

The Executive Committee of the Conference consisted of the following:

Prof. Hans Bender (Germany), Dr John Björkhem (Sweden), Dr Jan Ehrenwald (U.S.A.), Prof. H. Th. Fischer (Netherlands), Haakon Forwald (Sweden), Gabriel Marcel (France), Dr Marcel Martiny (France), Dr Gardner Murphy (U.S.A., *Chairman*), Dr Michel Pobers (U.S.A., *Secretary General*), Prof. H. H. Price (Great Britain), Prof. Emilio Servadio (Italy), Dr S. G. Soal (Great Britain), René Sudre (France), Prof. W. H. C. Tenhaeff (Netherlands), Dr R. H. Thouless (Great Britain), Prof. Hubert G. Urban (Austria), René Warcollier (France), George Zorab (Netherlands).

The following is a list of the Members of the Conference who were present in Utrecht:

Mrs Edward W. Allison (U.S.A.), Robert Amadou (France), Prof. Hans Bender (Germany), Dr John Björkhem (Sweden), Dr Gotthard Booth (U.S.A.), George Spencer Brown (Great Britain), Dr Hilde Bruch (U.S.A.), J. G. van Busschbach (Netherlands), Dr Wilfried Daim (Austria), Mrs Laura Dale (U.S.A.), Dr E. J. Dingwall (Great Britain), Dr Jacques Donnars (France), Dr Jan Ehrenwald (U.S.A.), Dr Jule Eisenbud (U.S.A.), Prof. H. Th. Fischer (Netherlands), Antony G. N. Flew (Great Britain), Haakon Forwald (Sweden), Prof. Gebhard Frei (Switzerland), Jean Gebser (Switzerland), Dr William H. Gillespie (Great Britain), Mrs K. M. Goldney (Great Britain), Prof. Hornell Hart (U.S.A.), Prof. Gustav Hartlaub (Germany), Prof. Peter Hohenwarter (Austria), Dr Betty Humphrey (U.S.A.), S. David Kahn (U.S.A.), Dr J. Kappers (Netherlands), Dr W. Kat (Netherlands), Raphael Kherumian (France), Dr Ir. J. M. J. Kooy (Netherlands), Prof. Sven

Krohn (Finland), Dr William Mackenzie (Italy), A. Mak (Netherlands), Gabriel Marcel (France), Dr Marcel Martiny (France), Dr R. A. McConnell (U.S.A.), Dr Joost A. M. Meerloo (U.S.A.), Dr C. A. Meier (Switzerland), Prof. Heinrich Meng (Switzerland), A. M. J. Mitchell (Great Britain), C. W. K. Mundle (Great Britain), Dr Gardner Murphy (U.S.A.), Dr H. Musaph (Netherlands), J. Fraser Nicol (U.S.A.), Edward Osborn (Great Britain), Claude Palmer (Great Britain), Denys Parsons (Great Britain), Prof. C. A. van Peursen (Netherlands), Dr J. J. Poortman (Netherlands), Mrs Dorothy Pope (U.S.A.), Prof. H. H. Price (Great Britain), Dr W. D. Rasch (Germany), W. G. Roll (Great Britain), Dr Louis Rose (Great Britain), W. H. Salter (Great Britain), Prof. Hans Schaefer (Germany), Dr Gertrude Schmeidler (U.S.A.), Michael Scriven (Australia), Prof. Emilio Servadio (Italy), Aage Slomann (Denmark), Dr S. G. Soal (Great Britain), Prof. Stefano Somogyi (Italy), Prof. F. J. M. Stratton (Great Britain), René Sudre (France), Prof. W. H. C. Tenhaeff (Netherlands), Dr R. H. Thouless (Great Britain), Dr G. G. M. Tjaden (Netherlands), Prof. S. W. Tromp (Netherlands), Dr Montague Ullman (U.S.A.), Prof. Hubert Urban (Austria), Dr Paul Vasse (France), Mme Paul Vasse (France), René Warcollier (France), Dr G. D. Wassermann (Great Britain), Dr Thorstein Wereide (Norway), G. Zorab (Netherlands).

OBITUARY

THE REV. C. DRAYTON THOMAS

THE Rev. C. Drayton Thomas, who died in July, joined the Society more than fifty years ago, and was therefore at the time of his death one of its senior Members. He became a Council Member in 1934 and was an active Member of Council until his death. He was one of the earliest and most regular sitters with Mrs Leonard, on whose mediumship he contributed numerous articles to our *Journal* and *Proceedings*, as well as making her the subject of his books *New Evidence for Human Survival* (1922) and *Life Beyond Death* (1928). He kept full and annotated records of his sittings with a care none too frequent among sitters, and generously made these available to other investigators. He also had gramophone records made of some of the communications from his father John, who like himself was a Methodist minister, and presented copies to the Society. The early sitters with Mrs Leonard were fortunate in having to deal with a medium and Control who fully entered into the spirit of scientific investigation, and indeed themselves proposed various methods of research which would provide crucial tests as to whether or not the content of the communications could reasonably be assigned to telepathy.

Among the methods suggested at an early date were book and newspaper tests. Mr Drayton Thomas was particularly interested

in these and contributed articles on them to the *Journal*, Vols. XX and XXII. I had in my own sittings some notable examples of book tests, which present a curious problem, not easy of solution. An analysis of this type of evidence will be found in an article by Mrs Sidgwick, *Proceedings*, Vol. XXXIII.

Mr Drayton Thomas was also much interested in proxy sittings, which were designed to exclude as far as practicable telepathy from persons present at the sitting. His first paper on this subject was published in our *Proceedings*, Vol. XLI, 1932-3, and he followed this up with the Bobbie Newlove Case, published in Volume XLIII, and another case in Volume XLV, which he undertook at the request of Professor E. R. Dodds, whose critical attitude to Spiritualist phenomena is well known. Professor Dodds commented on the case as follows: 'It appears to me that the hypothesis of fraud, rational inference from disclosed facts, telepathy from the actual sitter and coincidence cannot either singly or in combination account for the results obtained.'

When Whately Carington entered upon his elaborate 'Quantitative Study of Trance Personalities', Mr Drayton Thomas actively co-operated, giving Carington full opportunity to use as material his sittings with Mrs Leonard. He did not, however, agree with Carington's finding that Feda had no existence independent of the medium, and in *Proceedings*, Vol. XLIII, he vigorously contested this conclusion, setting out evidence that Feda was what she claimed to be.

While Mrs Leonard's mediumship was the main subject of his interest, he also concerned himself with many other aspects of psychical research; for instance, when Harry Price reported to the Society his exposure of the spirit photographer, William Hope, Mr Drayton Thomas showed himself very critical of Price's conduct of the experiments and his conclusions. During the Second War Mr Drayton Thomas was living for several years in the North of England and not in close touch with his colleagues in the Society. On his return to London it seemed to many of them that he had become less critical about the dubious elements in such phenomena as the 'Direct Voice'.

His long and close connection with the Society and his membership of the Council for nearly twenty years are a standing justification of the Society's tolerance of the most diverse opinions, and a proof that cordial and fruitful co-operation is possible between persons seeking the truth, though holding different views. His kindly and genial personality made him a very pleasant colleague whom I am glad to remember.

H. DE G. S.

REVIEWS

THE IMPRISONED SPLENDOUR. By Raynor C. Johnson. London, Hodder & Stoughton, 1953. 424 pp. 25s.

The author of this remarkable book is a physicist who is now Master of Queen's College in the University of Melbourne. His book deals with natural science, with psychical research, and with religious mysticism. He accepts as true the findings of all these branches of knowledge, and considers what we can infer from them as to our environment, our nature, and our destiny. It goes without saying that his treatment of natural science shows the accurate and scholarly knowledge of an expert in this field. These qualities of accuracy and scholarship are no less displayed when he proceeds to the fields of psychical research and mysticism.

The account given of experimental parapsychology is a very adequate summary of recent work in this field. The opening chapter on 'Telepathy and Clairvoyance' gives a survey of recent experimental work including that of Rhine, Tyrrell, and Soal and discusses various attempts at theoretical explanation such as the K-theory of Whately Carington. This is followed by a very good account of the experimental evidence for precognition and retrocognition and a discussion of the theoretical implications of the results of these experiments. The account of psychical research is completed by consideration of the less experimental aspects of the field in hauntings, poltergeist phenomena, and evidence for survival.

Discussion of parapsychology leads to a consideration of the evidence as to the nature of the universe derived from mystical experience in which the author uses both Christian and non-Christian sources. It is rare to find a scientist who fearlessly accepts the possibility that there are other ways of attaining knowledge than the way of science, and who resolutely accepts the consequences of this possibility. The author does not suppose that we can give satisfactory answers to all the questions which he has examined, but he finds that even to look fearlessly is to be strangely comforted. 'We live', he says, 'in a universe more wonderful than all our dreams: this at least seems clear. The faint and far-off voices that come to us seem wholly friendly.' And a little earlier: 'Love must be the greatest of all things, if through it and because of it God created all that is, so that we might in the end illustrate His perfection.'

R. H. THOULESS

CHALLENGE OF THE UNKNOWN. By LOUIS K. Anspacher. London, Allen & Unwin, 1952. 331 pp. 16s.

This is a British edition of a book that was published in the

United States in 1947. A short supplementary chapter has been added since Dr Anspacher's death, and there is an introduction by Waldemar Kaempffert. The book is not an investigation of psychic phenomena, but a more general and discursive setting-out of a philosophy of life, supported by illustrations from psychical research.

Part I is concerned with techniques of approach, and contains an interesting section on the evidential value of the trivial. The author adopts a reasonably balanced approach to Spiritualism, but suggests that the trivial nature of so many messages is of greater value as a means of identification than more elevating discourses would be. Part II, on psychic manifestations in art and literature, is a good catena of illustrations of how inspiration has come to great artists, and appears to have been compiled independently of Rosamond Harding's *An Anatomy of Inspiration*. Part III is headed 'The Present Verdict of Science on Psychic Manifestation', but the title is misleading. Much of the chapter is very general in scope, and the rest is concerned with the opinions of individual scientists, who are favourable, rather than with the trend of science as such.

Part IV gives the opinions of philosophers from Heraclitus to Santayana, while the closing Part interprets the Bible as a psychic document, in the light of such things as the medium of Endor, visions, xenoglossy, etc.

Dr Anspacher was closely associated with Professor J. H. Hyslop, and he has read widely. Yet not all of his citations are of equal worth. For example, on pp. 47 ff. he produces Clever Hans and the Elberfeld horses, as though their alleged feats of mathematics and spelling were unquestioned. Yet 'Clever Hans' has passed into the psychological vocabulary as an example of a misinterpretation of apparent facts, and the Elberfeld horses are not accepted at their face value by such an authority as David Katz. (See his *Animals and Men*, Longmans Green, 1937.)

It is strange that, in the pages on 'psychometry', Osty is mentioned only as the writer of an article in 1914, and no reference is made to his book in French and English. This book has an important bearing on the Spiritualistic hypothesis. Similarly, it is left to the supplementary chapter to call attention to Saltmarsh's book on Cross-correspondences, where Anspacher has referred to one instance only.

Occasionally there is some loose writing, as on p. 315; '... psychic levitation and telekinesis are the only ways to account for several of the miracles' (i.e. of the Bible). Levitation and tele-

kinesis, which are merely descriptive names, cannot 'account for' anything.

J. STAFFORD WRIGHT

THE JOURNAL OF PARAPSYCHOLOGY. Vol. 17, No. 2, June 1953.
Durham, N.C., Duke University Press. \$1.50.

Louisa Rhine's article on 'Subjective forms of spontaneous psi experiences' discusses the categories into which spontaneous accounts of psi experiences fall. This is part of the research now being undertaken at Duke Parapsychology Laboratory into their large files of spontaneous cases. She finds that most of the cases studied fall into the classes: (i) Intuitive, (ii) Hallucinatory, (iii) Unrealistic dreaming, (iv) Realistic dreaming.

Dr Pratt gives an account of position effects in the ESP data obtained by Dr Soal. These are of unexpected kinds, such as a significant difference between odd and even numbered trials, which suggests to the author a general tendency on the part of the subject to pattern ESP successes.

'Some experiments in precognition' is an account of experiments done by E. W. Bastin and Miss Green at Idaban, Nigeria. The method of experimenting was unusual in that the subject took a relatively long time in making his decision. An encouragingly high rate of scoring was found. There appeared to be a falling off in accuracy if checking was delayed for 15 minutes although the significance of this difference was not great.

There are letters from J. R. Swanton and C. W. K. Mundle, and a review by F. W. Knowles of *Der Zufall und das Schicksal* by W. von Scholz.

R. H. THOULESS

JOURNAL OF THE AMERICAN SOCIETY FOR PSYCHICAL RESEARCH.
Vol. 47, No. 3, July 1953. New York, A.S.P.R. \$1.50.

The major contribution is an article by Gardner Murphy on 'The Importance of Spontaneous Cases', under which heading Murphy includes spontaneous telepathy and precognition, apparitions, and spontaneous physical phenomena such as unexplained raps, lights, and movements of objects. He reviews the theories of spontaneous cases proposed by F. W. H. Myers, by G. N. M. Tyrrell, and by Hornell Hart, and describes Tyrrell's 1942 Myers Memorial Lecture on Apparitions as 'an amazingly close-thought, brilliant, integrated, all-around consideration of the problem'.

Dr Murphy announces that the American Society are starting a new and comprehensive study of spontaneous experiences with

emphasis on well-described cases, on recent cases, and on cases where there is confirmation. This is good news indeed, and we may be assured that the project will be attacked with lively determination and efficiency since the Committee consists of Professor Hornell Hart, Dr Gardner Murphy, Mrs Allison, Professor Ducasse and Mrs Dale.

Milton Millhauser gives an account of the interest in psychic matters of Robert Chambers, the founder of *Chambers's Journal*. Chambers (1802-1871) was converted first to Spiritualism, then to Episcopalian orthodoxy.

Gerald Heard reviews C. J. Ducasse's *A Philosophical Scrutiny of Religion*; C. J. Ducasse reviews Father Thurston's *The Physical Phenomena of Mysticism*; Dr J. B. Rhine reviews D. H. Rawcliffe's *The Psychology of the Occult*; and E. K. Schwartz reviews J. Hettinger's *Telepathy and Spiritualism*.

DENYS PARSONS

CORRESPONDENCE

PROBABILITY AND PARAPSYCHOLOGY

SIR,—In the July 25th issue of *Nature* (Vol. 172, No. 4369), there is an article by G. Spencer Brown entitled 'Statistical Significance in Psychical Research', in which he argues that significant results in Psi experiments may be due, not to some paranormal effect, but to unsoundness in the normal statistical methods. With this main contention I do not propose to deal, but Mr Brown uses a subsidiary argument, that any statistical PK experiment depends for its validity on randomization, and that such randomization is invalid if it might be influenced by PK; and therefore no such experiment can demonstrate PK (sixth paragraph).

This argument is surely unsound. Let us assume, for the sake of argument, a highly significant dice-throwing experiment in which all possibilities other than chance and PK are excluded. Then either PK did not affect the randomization, in which case the significance test is valid and the results must have been due to PK; or the randomization was affected by PK, in which case PK must exist. To put it differently, any significance test is a sort of 'reductio ad absurdum' argument, and must start off with a null hypothesis which is to be disproved: in this case, that there is no such thing as a PK effect. One is then entitled to assume the truth of the null hypothesis in carrying out the randomization.

It should be noted that while Mr. Brown's objection does not apply to an experiment to demonstrate PK, it does apply to an

attempt to measure its intensity, or to demonstrate subsidiary effects ; for then there is no null hypothesis denying the existence of PK, and so one may not assume this for randomization purposes. His objection also applies to his own experiment of matching randomly selected columns of digits in a table of random numbers, for his null hypothesis is simply that these columns are statistically independent. Thus his extremely interesting results may, perhaps, be due to PK. The objection might be minimised if the first two columns of the table were compared, with no randomization.

Mr Brown's revolutionary thesis is clearly of the utmost importance for Psychical Research, and deserves thorough investigation.

A. M. WESTERN

London, N.W. 3.

SIR,—The argument from my recent paper in *Nature*¹ can be put briefly as follows.

There are two and only two ways in which a test of statistical significance can be shown empirically to be invalid. In similar experiments, either statistically significant deviations must reappear with remarkable inconstancy, or statistically insignificant deviations must reappear with remarkable constancy. The former of these contingencies seems to be indicated in the field of psychical research at least, so that it becomes incumbent upon us to question the validity of the statistical test involved. When this is done it is seen that the calculus for the test uses an axiom of randomness² which can be shown³ to be without any precise empirical meaning. Thus it seems that our questioning was justified and that we might have to devise a new calculus.

It is not my contention that there never could be statistical evidence for PK ; the point I am concerned to make is that the evidence we have so far is not of the right sort. Mr Western does not say how we are to distinguish empirically between what he calls 'chance' and what he calls 'PK'. Until it has been shown that, on the nature of the present evidence, such a distinction can be made, the argument in his second paragraph applies to no known contingency.

The cross-check argument for the validity of some of the conclusions drawn from experiments in telepathy is not as like plane

¹ clxxii, 1953, 154-5.

² Richard von Mises, *Probability, Statistics and Truth* (London, 1939) ; Ronald A. Fisher, *The Design of Experiments* (London, 1947).

³ William Kneale, *Probability and Induction* (Oxford University Press, 1949).

sailing as Mr Mundle would have us suppose.¹ Dr Soal's first significant results with Mr Shackleton were obtained by means of what would be called, on Mr Mundle's own definition, a series of sixteen cross-checks.² Four of these gave significant deviations. There might be some justification for excluding these results from the field covered by the term 'cross-check' if in the next experiment they had been repeated. Here, however, significant matching was found in only one of the positions tried in the former series.³ We are thus at least left with three cross-checks which are inexplicably significant in this work alone. The fact that they are not called cross-checks but are cited as evidence for ESP is the result of a policy whereby, when there is reason to believe other causal agents have been eliminated, *all* statistically significant results are taken as evidence for ESP, and only non-significant results are cited as relevant to the applicability of the probability calculus used. But this is clearly begging the question at issue.

To demonstrate a causal tendency we must be able to say beforehand what sort of results we are going to get; that is, we must not only be able to predict 'significant' deviations one way or another, but also we must be able to say *which way* the deviations will go, and under what conditions they will occur and under what other conditions they will not occur.* If we cannot do all these things but still get 'significant' results, then logic demands that, whatever else we might question, we question also the applicability of the criterion of significance which we have been using.

G. SPENCER BROWN

Christ Church, Oxford.

PRECOGNITION, ESP, AND PSI-POSITIONING AS CAUSES OF PK EFFECTS

SIR,—The writer concluded in 1951⁴ that the results of all PK tests published up to that time could have been produced by (i) Precognition of the future falls of the experimental objects and the selection of targets that corresponded with these falls, (ii) a favourable method of recording the data, and (iii) a favourable method of subdividing the experiment, and that an experiment in pure PK, one in which Precognition is completely precluded, had yet to be performed. After this paper had gone to press, an

¹ *Jnl. S.P.R.*, xxxvii, 1953, 179–80.

² *Proc. S.P.R.*, xlv, 1942, 152–98.

³ *Proc. S.P.R.*, xlvii, 1943, 21–150.

⁴ C. B. Nash, 'Psychokinesis reconsidered.' *Jnl. A.S.P.R.*, xlv, 1951, 62–8.

experiment by Thouless¹ was published in which the order in which the six die faces were used as targets was determined almost completely at random. It was concluded that, even if the future falls of the dice had been precognized, the targets could not have been selected to correspond with them, and that, therefore, the significant deviations obtained in this experiment were not caused by Precognition but were manifestations of P K.

While in Thouless's experiment the order in which the faces to be used as targets was randomly determined, the actual targets were not selected at random as the number of die falls per target was arbitrarily set at 24 or twelve. Precognition of both the randomly determined order of targets and the uppermost faces of the die falls would permit selection of a particular number of die falls per target that would cause extra-chance agreement between the future die faces and the future targets.

It might be contended that such a method would necessitate Precognition of events that do not occur, as the number of actual die falls and the number of actual targets were the same and, to permit selection of a favourable fitting of one to the other, the number of precognized die falls and/or the number of precognized targets would have to be in excess of the number in the experiment. However, die falls and targets have been used in tests by Thouless since this experiment and the precognized die faces and/or targets not used in this experiment could have occurred in the succeeding test.

In addition to a significant deviation of target hits in Thouless's experiment, there were also a significantly higher score in the first runs of the test occasions and a significant difference between the scores of morning and afternoon test occasions. These effects could have been produced by precognitive selections of favourable numbers of runs for the test occasions as the length of the experimental subdivisions was not randomly determined.

As the significant deviations of Thouless's experiment could have been produced by Precognition, they do not constitute direct evidence of the existence of P K. In the writer's opinion none of the P K experiments published since Thouless's experiment contain direct evidence of P K. The target scoring in these experiments could have been produced by Precognition as the number of die falls per target was not randomly determined, and they contain no other effects not accountable for by Precognition.

ESP may also be the cause of significant effects in certain P K

¹ R. H. Thouless. 'A report on an experiment on psychokinesis with dice, and a discussion on psychological factors favouring success.' *Proc. S.P.R.*, xlix, 1951, 107-30.

tests. Favourable analysands or favourable experiments for declared analysands may be selected by ESP of the recorded data, unless the analysands were specified before the performance of the experiment. While such analyses are not without value, significant results obtained by them do not constitute direct evidence of PK.

It is possible that dice could be given initial positions favourable to the production of a particular terminal effect. Favourable positioning might be done unconsciously by normal processes, e.g. by cognizing the favourable die axis and placing the die in a position which would result in its beginning its roll on this axis, but the occurrence of such normal processes is highly questionable. There is less doubt, however, that favourable positioning could be accomplished by paranormal processes such as psi-prediction¹ of a favourable position and PK-placement of the die in this position.

Favourable positioning was precluded in the experiment of Thouless by always placing the dice in the same initial position. The writer believes that this is unnecessary and that the use of a die container in which the dice have to be dropped and which prevents favourable positioning other than by the use of psi is sufficient for the following reasons. The published data of Thouless's experiment show that the maximum deviation of any die face was 6.25%, which is equivalent to the deviation that would result from favourable positioning in every trial. From the percentage occurrence of psi effects in this and other experiments, favourable positioning of the dice by psi would not be frequent and favourable positioning without the use of PK (in most PK experiments there is no reason to preclude PK effects) would be even less frequent. As there is no evidence that psi-positioning occurs, a value of 10% is probably in excess of its actual frequency. Even with favourable positioning by psi occurring in 10% of the trials, however, it would produce a deviation of only 0.625% on the basis of Thouless's results. A deviation of this magnitude would not be significant in PK tests.

It is of interest to consider in what initial positions dice could be placed to produce a terminal effect. One position is that in which they will begin their roll on an axis favourable to the production of the desired target. In Thouless's experiment the faces on the ends of the axis used at the beginning of the roll had an average deviation of -5.1% . This had the effect of causing an average deviation of $+2.6\%$ for each of the other four faces. Thus, axis-positioning under the conditions of his experiment

¹ Psi-prediction, or ESP-prediction (Nash, op. cit.) is prediction with the use of psi faculties other than Precognition of the predicted effect.

would increase the score of the accepted face by 2.6 % and decrease the score of the rejected face by 5.1 %.

Another initial position in which dice could be placed to produce a terminal effect is that position wherein the distance from their average stopping point will cause the accepted face to land uppermost. Thouless used the 3-4 axis with the 2 face uppermost which, in so far as axis-positioning alone is concerned, causes equal positive deviations of the 1, 2, 5, and 6 faces. When the effect of use of the 3-4 axis is corrected for, these faces have the following deviations: 1 = +3.7 %, 2 = +0.7 %, 5 = -0.5 % and 6 = -3.9 %. These deviations could have resulted from dice bias and, as they are not significant, they could be random results. However, they could also be the result of the distance of the initial position of the die face from its average terminal position. If the 1 face was most favoured by distance-positioning, as its deviation suggests, it follows that the adjacent faces, 2 and 5, will have a smaller deviation than the 1 face and that the opposite face, 6, will have a larger negative deviation than either adjacent face. As the deviations are in agreement with this prediction, they suggest that, under the conditions of Thouless's experiment, distance-positioning of the dice would change the score of a die face by as much as 3.9 %.

As axis-positioning is twice as effective in decreasing the score of a die face as it is in increasing the score, and as distance-positioning is equally effective in decreasing or in increasing the score of a die face, psi-positioning would be expected to be more effective in decreasing the score of a die face than in increasing it.

CARROLL BLUE NASH

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COSMOLOGY AND PSYCHICAL RESEARCH

SIR,—I have read Mr Basil Smith's letter (*Journal*, Vol. 37, No. 676) with interest. I do not understand, however, what he means by the expression 'space-time' *vis-à-vis* ESP. It is possible that telepathic communication and the radiation of light possess certain properties in common. If telepathic experiments could be devised (with our present knowledge this is impossible) not dissimilar from the Michelson-Morley light experiments, it might be that their results would be the same. That 'those dreams which are fulfilled within a shorter period outnumber those which are fulfilled within a longer period' (if such has been statistically established) may be explained, I think, on other grounds. If it

was possible to devise an experiment whereby percipients were placed at intervals of 500 miles longitudinally from one another (the first percipient to be placed that distance from the agent) it would be quite impossible, I think, to determine whether any communication had 'weakened', since there would be no standard of perspicacity in each percipient. Apart from this consideration, little is known of the length of time, which may vary for emotional and other reasons, before an 'impression' emerges into the conscious, which constitutes the only evidence of its reception.

My primary purpose in continuing this correspondence is to enquire whether any serious attempt is being made to resolve what I may call the cosmological problems arising from psychical research. It would appear that many of such problems have been neglected. I suggest, in alleged 'spirit communication', that *ad hoc* experiments should be devised (it is preferable that the 'medium' should possess little knowledge of the subject) whereby enquiries may be addressed by and replies sought from those likely to understand one another in scientific terminology. *Consensus ad idem* is essential and I am not unmindful of the fact that to understand 'one another' presents a major problem. Many enquiries, including the following, will suggest themselves: Are you able to measure time? If you are, from what position extraneous to the earth and by what method do you do so? Do you make appointments with other 'spirits' to 'meet them', are you and they able to keep such appointments and if you are, by what method do you do so? Does your 'meeting' appear to you to be an environmental process external to yourself? Are you able to 'see' our sun and our moon? (This enquiry should not be difficult to answer, although it would not necessarily follow that what is 'seen' is our sun and moon.) Are you able to measure distance? If you are, by what method do you do so? What do you understand by space? What do you understand by matter? Are you able to distinguish between matter of which your previous environment was composed and 'matter' of which your present environment may 'appear' to you to be composed? If you are, by what method do you do so? What do you understand by movement? Do you have periods of 'unconsciousness' when you are unaware of your 'existence'? If you do, in what circumstances does such a condition arise and when 'consciousness' has been regained, is your immediate environment unchanged?

That 'spirits' continue or do not continue to associate themselves with their previous environment, is a self-evident proposition. From the postulate that 'spirits' continue to associate themselves with their previous environment (as Spiritualists claim that they

do) we may deduce that they are either unwilling or unable to disassociate themselves from that environment. The former condition suggests a selective and purposive association and the latter condition suggests a mechanistic association between 'mind' and 'matter' of a reciprocal nature. Both conditions suggest that 'mind' and 'matter' may be inextricably interwoven threads of a single garment and that their ultimate synthesis may reveal a teleological process in the final destiny of man. That the dispersal and distribution of 'spirits' throughout the universe (if they can be said to occupy space) are capricious and are fortuitous, is a proposition that is not only unsatisfying, but unacceptable to our intellect. Nevertheless, hypotheses of an epistemological nature, sought to establish the validity of design and purpose in the universe and an ontological conception of that part of our 'personality' that may survive death, are, in essence, extrapolatory. To 'spirits' (if they do not remain contiguous to the earth) occupying vast inter-stellar space, deprived of observational data, cosmological (quantitative) time, distinguished from psychological (qualitative) time, is incomputable; astronomical constants, deduced from parallax measurement, are devoid of meaning and incomprehensible; time, objectively yet artificially derived from the axial rotation of the earth (night and day) and the revolution of the earth around the sun (the seasons) no longer serves that useful purpose. The sun, a familiar and empirical landmark in a hitherto and necessarily geocentric and anthropocentric cosmology, is no longer her trusted and inseparable companion.

I suggest that evidence of survival will remain inconclusive, until some knowledge of the nature, dimensions, and boundary of the 'spirit world' is communicated to us in a manner which is consistent and which we can comprehend.

DENIS CHESTERS

Brighton, Sussex.

ESP ABILITY AND HORMONE TREATMENT

SIR,—The letter in your May-June number by Professor T. T. Beck raises some interesting social and ethical questions. The author believes that, if we accept as a fact the greater paragnostic perceptiveness of women as compared with men, it might be worth while trying to find out whether the psychic power of a man could be developed by the use of some kind of female hormone. One naturally wonders what personality-changes might be involved in such a process, and how far they might incur the hostility of the conservative.

May it be that Professor Beck's idea was what underlay the

little-understood practices of many primitive tribes? I have read, in some anthropological book whose title at the moment escapes me, of tribes in Siberia whose Shamans were believed to possess the gift of prophecy and other psychic powers only so long as they completely renounced the ordinary life of the male: dressed and treated in every way as women, they shared every possible aspect of feminine drudgery in order to maintain contact with the source of their inspiration.

Is it possible that, apart from any actual physical mutilation such as that which the priests of Cybele had to undergo, the mere sharing of diet and work and interests and personal status with the women might induce in a man psycho-physical changes such as those produced by hormone-injections?

G. B. RIDDEHOUGH

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University of British Columbia.

'SURVIVAL AND THE IDEA OF "ANOTHER WORLD"'

SIR,—'People are what you meet'; the correspondence in the last three numbers of this Journal on Professor Price's paper in Part 182 of the *Proceedings* prompts me to put forward the following thoughts.

Professor Price points out that a person cannot be 'met' whilst dreaming (p. 10). I should like to raise the question as to how far the 'I' when dreaming am the same as 'I' when awake. The dreaming 'I' has thoughts, and images actions, which seem inconsistent with the character of the waking 'I'. Inconsistent, that is, to the casual observer; the insight of the psychologist into the dreamer's subconscious would reveal a basic unity between the two. But to the untrained observer, the only justification for saying that the two 'I's' are the same is that both have relations with the same brain and material body.

But if the personality of dreaming 'I' is superficially so different from that of waking 'I', is it not reasonable to suppose that if survived spirit 'I' exists, it might appear entirely different from either; and that just as we must use psychology to demonstrate the underlying consistency of the two views of the personality of the living person, so we may need more than psychology or insight to prove the identity of two personalities separated by time and death? If so, the only possible meaning of survival would be a *personal* one; an experience of continuity of consciousness similar to that in falling asleep.

Incidents of waking life may intrude themselves into dreams; but the dreamer is generally concerned with his private dream world. We need not suppose that a survived spirit necessarily

remembers things which occurred to the *ante mortem* personality (or experiences any desire to communicate them to us) any more than we need suppose our dreams to be necessarily based on memories of waking life. There is more likely to be correspondence, as Professor Price is at pains to point out (*Jnl. S.P.R.*, xxxvii, 172), between *post mortem* experiences and 'wishes (conscious or repressed) which were characteristic' of the *ante mortem* personality, just as many dreams consummate the expressed or subconscious desires of the dreamer. But this criterion is so vague as to be almost valueless. Basically, the number of psychological drives which determine our behaviour and our dreams is a small one. We can hardly expect post mortem communication to be so sensitive as to reveal quantitatively significant features of these basic urges ; it is as bad as trying 'to guess the author of a coded telegram by his style' (cf. G. N. M. Tyrrell, *Proc. S.P.R.*, xlvii, 255).

Thus it seems to me that, in view of possible alterations in personality on death and the difficulty of recognizing a personality by the character of its fulfilled *ante mortem* desires, survival would mean nothing to anybody until he had experienced it, and its experience would be its only proof—and that an incommunicable one.

MICHAEL C. PERRY

Trinity College, Cambridge.

THE PSYCHO-ANALYTICAL THEORY OF POLTERGEISTS

SIR,—In the May-June issue of the *Journal* was published a review by Dr J. C. Flugel of *The Story of the Poltergeist down the Centuries* by Hereward Carrington and Nandor Fodor. This review may have given the impression that the psychoanalytical theory of Poltergeists was published for the first time in that book. Therefore we beg to inform you that Dr Alfred Winterstein (Vienna), former President of the Austrian Society of Psychical Research, published a paper, delivered at the Athens Congress in 1930, under the title 'Un Cas de Hantise Médiumnique (Frieda Weissl)' where he developed his psychoanalytical theory of Poltergeists, published earlier in *Imago XII* (1926) H.2/3 and *Zeitschrift für Parapsychologie* (September 1926) under the titles : 'Zur Psychoanalyse des Spuks' and 'Psychoanalytische Bemerkungen zum Thema Spuk'.

Also Countess Zoë Wassilko-Serecki (Vienna), Hon. Secretary of the Austrian Society for Psychical Research, published some similar views on Poltergeist phenomena in her articles 'Beobachtungen an Eleonore Zugun' (*Zeitschrift für Parapsychologie*,

February 1927) and 'Observations on Eleonora Zugun' (*Journal of the American Society for Psychical Research*, XX, September-October 1926).

ZOË WASSILKO-SERECKI

Vienna.

ANTOINE RICHARD'S GARDEN

SIR,—In Mr Lambert's very interesting account of further mysteries relating to *An Adventure* he says (*Journal*, July-October, p. 142) that he considers it unlikely that de Croÿ made use of Chambers's design before the latter's visit to Paris in 1774, and adds that he has not succeeded in discovering the dates of Chambers's arrival and departure.

From his letter-books in the British Museum, which have not yet been printed, it is clear that he must have arrived in Paris between May 1 and about May 5 since a letter from his daughter, dated May 8, says that she is glad that 'he passed through the voyage so easily'. He left Paris on the evening of May 30 and later, writing to Worsley on June 16, says that he met nobody in France who knew the King, so that it is clear that he did not see de Croÿ when he was in Paris.

E. J. DINGWALL

Cambridge.

SIR,—It has always been my view that the only ground on which the experience at the Petit Trianon recounted by Miss Moberly and Miss Jourdain could be dismissed is the ground of a calculated and consummately perpetrated fraud. Since the good faith of the narrators is universally admitted, I have never been able to understand how anyone could classify their experience as illusion—the misinterpretation of the normal surroundings. Therefore, I think that G. W. Lambert has done an important service in recalling attention in your *Journal* to a case which was once too summarily dismissed, and he is to be congratulated on his attractive presentation of a new theory based on close original research.

It appears to me that the discovery (that is, discovery in connection with this case) of Le Rouge's Plate No. 19 is extremely important. The barrier, the wood, and the approximately located kiosk make up together an undeniably strong combination. None the less, Mr Lambert does not convince me that he is interpreting his discovery correctly. He refers (p. 128) to 'a square site'. But the plan in Plate 19 clearly shows a square *building*, and indeed on

his next page (p. 129) Mr Lambert describes Plate 23, where the kiosk is depicted as a square building with a pillar at each corner. What is the connection between this square four-pillared building, surrounded by water, and Miss Moberly's drawing of a round building with about ten pillars, and no water surrounding it? The connection is that the roof in Miss Moberly's sketch of 1904 has a slightly Chinese look, that the two narrators in 1909 spoke for the first time of 'a Chinese effect', and that these two circumstances indicate a relation with Antoine Richard's 'projet' for a 'Jardin Anglo-chinois' as depicted in Le Rouge's Plate 19.

But if we examine the Key to the plan in Plate 19 we see that the objects in the projected garden were to be not only English and Chinese but Turkish, French, classical, etc. Since the word 'kiosk' is Turkish in origin, is it not too big an assumption that Richard must have envisaged this particular building as Chinese? He may have done so, but it seems to me that in a garden replete with so many objects 'exotiques' he might have pictured the kiosk as a great many things besides Sir William Chambers's Chinese conceptions; even though, as Mr Lambert shows (p. 141) in one design such a Chinese building was called a kiosk. Moreover, it is rather hard to believe that during the intensive research undertaken by the two narrators they did not find so important a publication as Le Rouge's plates. If they found Plate 19, it would be perfectly natural and not at all disingenuous on their part not to adduce as evidence a mere plan which did not relate to a real garden, but simply to a 'projet' of 1774. All the time they were looking for the realities of 1789, and would know that if they adduced the project of 1774 amused critics would promptly enquire how Marie Antoinette had managed to get into an environment once proposed but never apparently carried into effect. Besides, the narrators had discovered in 1909 a reproduction of a map of 1705 with 'Le Kiosque' marked. On this they commented: 'Therefore the mere chance name which from the first we gave to our building was justified by there having been something called by that name exactly in that part of the garden' (*An Adventure*, 1911, p. 50).

Whether they found Le Rouge's Plates or not, it seems more than probable that Plate 19 has a bearing on the original narratives of 1901. But the nature of the connection must be a matter of opinion. I would suggest that particular note has to be taken of the word '*Projet*', most prominently printed on the plan, a word which is suggestive of the projection of the mind or astral body; and also of the word '*Jardin*', which, though it has no etymological connection with '*Jourdain*', has what is psychologically more

significant, a strong visual and auditory resemblance to it. In my view, there cannot safely be excluded from the consideration of any case of this class the possibility of paranormal perception of books and other records. It will be recalled that Myers cited the case of Miss A, a young lady who in 1889 saw in Salisbury Cathedral a monk in a brown gown, and who afterwards found in Stevens's *Continuation of Dugdale's Monasticon* a plate of a Franciscan which correspond 'exactly' with what she had seen (*Proceedings S.P.R.*, viii, 507). That apparent retrocognition may be precognition of future research is a view which Mr Lambert thinks does not meet the Moberly-Jourdain case because of the 'complicated pattern of ideas and information, the significance of which may or may not be verified by subsequent research', and he prefers the agency of memories, desires, and images existing in the absence of a physical brain. The hypothesis of the influence of the mind of the gardener Richard certainly cannot be dismissed if one admits that any relevant significance at all attaches to his plan as we know it from Plate 19; but in introducing also the mind of the Duc de Croÿ has not Mr Lambert interpreted the story in *An Adventure* with much too great a freedom?

The narrators did not say that the man at the kiosk was suffering from smallpox. They said that he was 'marked' by smallpox. With memories extending back long before the days of compulsory vaccination, Miss Moberly and Miss Jourdain knew perfectly well what a man marked with smallpox looks like. By their words they certainly meant that their man was pitted with the disease, not that he was covered with the pustules. His darkness reflected the common delusion, fostered by melodrama and popular fiction, that all villains are dark men, and I hold that this part of the 'dream' of the two unmarried ladies is mainly if not entirely erotic symbolism. The Temple of Love, which the Baedeker they carried had told them to expect, must be equipped with its 'bad man', representing forbidden sex; and they are rescued by a wholesome young man, 'obviously a gentleman', and therefore, following clerical-parental indoctrination, incapable of impropriety. A little further back in the Baedeker are to be found hints of the dangers of Paris to ladies: 'When ladies are of the party an unmistakably first-class hotel should always be selected.' Observe that this objectionable kiosk man was not allowed to look directly at the ladies, for had he done so action might have followed, and such action was unthinkable. The *evil* of the man was what the two ladies stressed above everything. Can we believe that the loyal M. de Croÿ thought of his King as *evil*-looking, however repulsive his disease had made him appear? Would he envisage

the King, whom he had seen dying in bed, as wearing the large hat and flowing cloak of the kiosk man?

W. H. W. SABINE

New York.

With the permission of the authors, Mr G. W. Lambert has seen the two foregoing letters, and has furnished the following comments :

The information Dr Dingwall contributes is of definite value, and discloses a source of information which may repay further study. It is now clear that Sir William Chambers arrived in Paris only a few days before the death of Louis XV on 10 May 1774, an event which was obviously unexpected when he planned his visit ; and that he left again on 30 May. During his short stay there can have been no entertaining by French notabilities owing to court mourning, and it is to be assumed that Chambers only made contact with a few Anglophil Frenchmen (among whom the Duc de Croÿ certainly could not be counted) and with fellow architects. When Chambers, in a letter of 16 June said that he met no one in France who knew the King, he was presumably referring to the new King, Louis XVI. One can be certain, therefore, that Chambers did not meet de Croÿ, as Dr Dingwall observes, and that he did not discuss with anyone at Court the future plans for the garden at the Petit Trianon. Nevertheless, it remains a possibility that Chambers stimulated an interest in his designs when he was in Paris for a visit which happened by chance to fall just at the time when an 'English' garden was being projected by the new Queen. If a Chambers design was ever put forward at all for that garden, it was almost certainly proffered neither by Richard nor by de Croÿ, but by someone like de Caranman, who had the ear of the Queen during the summer of 1774.

I certainly did not intend, by my remarks on p. 129, to give the impression Mr W. H. W. Sabine attributes to me, that the two ladies saw young Richard's design for Site 7. That design was quite unlike the building in the sketch (Fig. 4). Its style, as Mr Sabine rightly guesses, was not even Chinese. I think it was meant to be Turkish. In relation to that feature the only 'coincidences' that can be claimed between the *Projet* of 1774 and the 'vision' of 1901 are the position in the garden and the name ('kiosk') of the structure. That the term 'kiosk' was used for Chinese as well as Turkish garden pavilions is proved by de Croÿ's design for his own garden (see p. 141).

As to whether the two ladies consulted Le Rouge's volumes during the course of their researches after the experience of 10 August 1901, an answer to this question may prove to be obtainable

from their notes preserved in the Bodleian Library. In his penultimate paragraph Mr Sabine considers that I have gone too far in introducing 'the mind of the Duc de Croÿ'. It is not by any means essential to my story to suppose that the two ladies 'tapped' the memories of the Duc. I made considerable use of his Journal, because it contained much detail very relevant to the history of the time. There is a strong presumption that old Richard, who was a trusted retainer, saw Louis XV more than once during his last illness, and if anyone's memory was 'tapped' during the experience, I should say it was that of old Richard. Whether there is any proof (or disproof) of my surmises to be found in the French archives of the time it is too early, as yet, to say. It seemed worth while publishing the story, before all possible sources of documentary information had been exhausted, in the hope of enlisting the help of others in the task of exploring the historical aspect.

In his final paragraph Mr Sabine expresses the confident opinion that when the ladies said the man at the kiosk was 'marked' by smallpox, they meant that his face was pitted with the disease, knowing perfectly well what a man so marked looked like. He then goes on to give that part of the 'dream' an explanation on psychoanalytic lines. As to the first point, I would repeat that only one of the two ladies (Miss Jourdain) mentioned smallpox at all, and that was not in her original narrative. The impression I get is that to begin with neither observer was very sure what caused the disfigurement of the man's face. Whether the outward appearance indicated active disease or an attack in the past, the reference to smallpox at all still seems to me curious in the circumstances. The look on the face might well have appeared 'evil' to a stranger, who could only judge from the repulsively coarsened features. As to the second point, it is true that the student of parapsychology must be continually on his guard against attributing to ESP 'material' which could more reasonably be attributed to ordinary subconscious processes. But why does Mr Sabine stop short at the kiosk? It would be logical to go further and to interpret the whole of the material in the same way, thus getting rid of any hypothesis of paranormal agency. If the kiosk represented 'forbidden territory' in the sense Mr Sabine suggests, what of the 'barrier'; of the 'second house', the door of which was slammed shut, and from which a man guided the two ladies away; and of the wood, in which tortuous paths led Miss Jourdain away from the directions in which she intended to go, and from the neighbourhood of which she was guided by a 'gardener'. Is not all this symbolism having the same significance?

At the risk of being told that my dissent is attributable to a

'resistance' of a psychological kind, I must record my inability to find a satisfactory answer in that direction. In the first place, this was a case of *collective* hallucination. As I observed on p. 153, two individuals would not have had projected into consciousness identical symbols at the same time, if their respective origins were in the 'private' subconscious of each. It is just thinkable that one of the two generated the images from her own subconscious, and that the other 'saw' them by an exercise of ESP, assisted by suggestion. But I am not aware of any other collective hallucination which can be so explained. Secondly, Mr Sabine's theory about this part of the experience is very difficult to square with the fact that the two ladies, who spent a great deal of time together in the course of their lives, had no other collective hallucinations, apart from the series connected with the Petit Trianon garden. Miss Moberly was somewhat 'psychic' and had other visual hallucinations, but in the experience of Miss Jourdain the Petit Trianon series was unique.

It may turn out to be impossible to prove beyond reasonable doubt that the imagery seen was in any degree veridical. On this point the reader would be well advised to suspend judgment until a later stage. But whether the imagery was veridical or not, it does seem to have been characteristic of the place rather than of the persons who saw it.

ANTOINE RICHARD'S GARDEN

ERRATA (JOURNAL, JULY-OCTOBER, No. 676)

P. 129, penultimate line, and page 134, line 21 : the references to Richard's Plan should be 2, not 3.

P. 153, fifth line from end : for 'into' read 'with'.

'APPARITIONS' RE-ISSUED

The Seventh Myers Memorial Lecture by the late G. N. M. Tyrrell, first published in 1943 and long out of print, is being re-issued with a new Preface by Professor H. H. Price. For easier reference, the sixty-one principal cases cited have been re-numbered ; an Appendix has been added, giving a complete list of them under Tyrrell's subject-headings, with sources and the numbers of the pages on which they are referred to in the book ; the headings in the text have been re-arranged and expanded ; and an index has been added. The book is bound in cloth and will be published in November at 12s. 6d. by Gerald Duckworth & Co. Ltd.